ical public of this country and of Europe, to assert its real value, and to point out the best methods of utilizing it. A quarter of a century has elapsed since its discovery. This period is long enough for the heat of partisanship to cool, and to afford an opportunity for an impartial statement, by an impartial observer, to be heard with calmness and interest. Moreover, the centennial anniversary of the nation's existence is an auspicious moment for putting on record the testimony of an intelligent and disinterested witness to the discovery of an agent in which the whole human race are interested. With the hope of obtaining from Dr. Bigelow a history of the discovery of anæsthesia, the following note was addressed to him:—

"Dr. HENRY J. BIGELOW, *Professor of Surgery in Harvard University.*

DEAR SIR: I am preparing a report on the progress of practical medicine in this country for the past century. In such an essay a notice of America's greatest contribution to medicine, modern anæsthesia, is indispensable. If you have the time and are willing to prepare a history of its discovery, with which you are so familiar, you will not only confer a favour upon all interested in it, but will put on record an authentic account of the discovery, by one who was an eye witness and actor in its early scenes, and to whose statements, on account of their disinterestedness, great value is attached."

To this letter the following paper was received in reply.

A HISTORY OF THE DISCOVERY OF MODERN ANÆSTHESIA.

MY DEAR SIR: A quarter of a century ago, your simple proposition would have re-awakened a discussion which had already exhausted the subject. Even so long ago as 1848, I thought it discreet to preface a paper upon the abstract question of discovery, as decided by historical precedent, with the disavowal of an intention to "dig up the well-worn hatchet of the ether controversy." But I see no objection to a review now—final, so far as I am concerned—of the occurrences you refer to, especially if I offer no opinion without its reason.

The singular persistence of the controversy was due to a variety of causes. People differ in their views about what constitutes a discovery or a discoverer. A voluminous mass of sworn testimony availed little in those days, for want of some machinery to reach and fix a decision based upon it. One of the contestants, at least, felt this, and vainly urged a court of justice.[1] Preponderating opinions and evidence were laboriously and repeatedly brought to the surface by Congressional committees, and by other bodies and committees of those most familiar with the circumstances; but, for the lack of a tribunal accustomed to estimate the weight and quality of scientific evidence, not to say evidence of fact, no absolute decision was reached. The result was, that every new discussion began and ended like the preceding, and to as little purpose.

When the discovery was announced (October, 1846), the circumstances were few and recent, and the details of its progress were known. But when history was obscured, when another State, and even another Nation, had set up each its own discoverer, and readers were perplexed with volumes of new reports and new testimony, it became less easy to sift the evidence. Claims, till then distinct, overlaid each other. Each alleged inventor, with his partisans, aimed to secure the whole honour. Opinions were pronounced by people who knew little of the facts. Attempts were even made, in more than one instance, to forestall or manufacture public opinion, by procuring in promiscuous medical assemblies and legislatures sudden votes designating some discoverer by name, with a view to influence the erection of statues. In a scientific view such votes are not worth the paper that records them; but it cannot be doubted that in a free country every citizen has the inherent right, of which the late Lord Timothy Dexter so liberally availed himself, to erect in his front yard a statue with an inscription.

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[1] See Congressional Debates for the sessions of 1853 and 1854.

The claimants to the discovery are three—Dr. Wells, Dr. Jackson, and Dr. Morton. In discussing their claims, we cannot overlook the fact that the discovery was equally possible to either of them, or indeed to any moderately ingenious person whose attention should have been directed to the subject. This greatest single step forward in the history of medicine, like the division of the Roman printing-block, was a very small advance in strictly scientific knowledge. Facts of insensibility to pain, produced both by gas and ether, were already known to the world. Art needed only an extension of their application; and so far as art or science was involved, either Wells, Jackson, or Morton was competent to the work.

This simple statement comprehends certain points of vital importance. The first essential requisite of modern anæsthesia is, that it shall be always attainable, and, when attained, complete. A second requisite is, that the insensibility shall be safe. The discovery embraced the threefold and essential novelties, that it is, under proper guidance, *complete*, *inevitable*, and *safe*, and not, like all previous stupefaction, partial, occasional, or dangerous. If only partial or occasional, or if dangerous, neither the patient, the dentist, nor the surgeon would consider it of value. Even so late as a year after the discovery, many surgeons, and, extraordinary as it may now appear, some hospitals, absolutely declined to use the new means of producing insensibility, on the ground that it was attended with danger.

Readers of the present day may not remember how surprisingly far previous knowledge of anæsthesia had extended. Although there has been a want of discernment in attempts to point out precisely what the new advance upon previous knowledge was, one great difficulty has been, that this advance was so small, in a strictly scientific view, that it was not easy to measure it. This difficulty was enhanced by the magnitude of the spoil, whether mere honour, or, as beyond all question it should have been, emolument.

A rapid review of the history of early anæsthesia will bring us to the period with which we are especially concerned.

In the anæsthetic state, the action upon the brain may be a primary one, as by its compression, or by hypnotism—or secondary, as by narcotic and inebriating agents absorbed into the blood, from the lungs, the digestive tube, the skin, or other tissues. A few extracts, abridged from the familiar literature of the subject, will show that surgical anæsthesia in these various forms has been long known and longer sought.

The use of poppy, henbane, mandragora, hemp, etc., to deaden the pain of execution and of surgery, may be traced to a remote antiquity. Herodotus ascribes to the Scythians the use of a vapour of hempseed to produce drunkenness by inhalation. In China, Hoa-tho, in the year 220, administered hashish (*Mayo*) and performed wholly painless amputations;

the patient recovering after a number of days. Hashish, described by Herodotus twenty-three centuries ago, is the active agent of the modern *Bhang* of India.

Pliny, who perished A.D. 79, says of mandragora: "It is drunk before cuttings and puncturings, lest they should be felt." Dioscorides gives an elaborate method of preparing mandragora to produce anæsthesia (*ποιεῖν ἀναισθησίαν*) in those who are to be cut or cauterized—" or sawed," adds Dodoneus, and who in consequence "do not feel pain." Half an ounce, with wine, says Apuleius, a century later, enables a patient to sleep during amputation "without sensation." "Eruca" was used by criminals about to undergo the lash. "Memphitis," a "stone," so "paralyzed parts to be cut or cauterized that they felt no pain."

Theodoric, about the year 1298, gives elaborate directions how to prepare a "*spongia somnifera*," by boiling it dry in numerous strong narcotics, and afterwards moistening it for inhalation before operations. In 1832, M. Dauriol, near Toulouse, cites five cases of insensibility during surgical operations, induced by him with the aid of a similar "sponge" It is said that persons unpacking opium have fallen suddenly.

In 1532, Canappe described the inhaling-sponge of Theodoric, and at the same time warned surgeons against giving opium (*à boire*), which "sometimes kills." But in later years, and until ether was introduced, it was the rule to give opium before operations.

September 3, 1828, M. Girardin read to the Academy of Medicine a letter addressed to his Majesty, Charles X., describing surgical anæsthesia by means of inhaled gases.

Richerand suggests drunkenness in reducing dislocations. Patients, while dead drunk, have been operated upon painlessly, and a dislocated hip was thus reduced after a bottle of Port wine. Haller, Deneux, and Blandin report like painless results in surgical and obstetric experience.

Baron Larrey, after the battle of Eylau, found in the wounded who suffered amputations a remarkable-insensibility, owing to the intense cold. Of late years congelation has become a recognized agent of local anæsthesia.

Hypnotism is a very remarkable cerebral condition, by which surgery has been rendered painless. It is the grain of truth upon which the fallacies of mesmerism, animal magnetism, and the rank imposture of so-called spiritualism have been based.

The experiments of the Grotto del Cane are familiar, as also are recoveries from accidental asphyxia after complete insensibility.

About a year ago, two healthy men, at my request, inhaled atmospheric air from a common gas-bag. As carbonic acid replaced the oxygen, they both became livid, and, to every external sign, utterly insensible. One was really insensible; the other nearly so, but, being a plethoric subject, it was deemed prudent in his case not to press the inhalation further.

Nitrous oxide after a time asphyxiates, owing to the chemical combination of its gases—on that account parting reluctantly with its oxygen. But it also exhilarates, and its anæsthesia is probably something more than a condition of asphyxia.

These facts show that from time immemorial the world has been in possession of an anæsthesia which was occasionally resorted to, and not unfrequently amounted to complete insensibility. But, as a rule, the following propositions held good in respect to it.

1st. It could not be relied on to affect everybody.
2d. It was often insufficient.
3d It was sometimes dangerous.

What surgeons and patients needed was an inevitable, complete, and safe condition of insensibility; and this they were soon to have.

The moment arrived. In three months from October, 1846, ether anæsthesia had spread all over the civilized world. No single announcement ever created so great and general excitement in so short a time. Surgeons, sufferers, scientific men, everybody, united in simultaneous demonstration of heartfelt mutual congratulation.

It is to be regretted that no single individual stood out clearly, at this time, to receive the homage and gratitude of the world.

Nothing like the new anæsthesia had been known before. Whatever has been devised since has been a mere imitation and repetition of this—I may almost say, with no single substantial advantage over it. Our English friends, with a pardonable pride in matters of scientific discovery, not unfrequently formulate their convictions thus: "A. had indeed shown this, and B. that; but it was reserved for our own C. to make the imperishable discovery." It is probable that the average Englishman still believes that modern anæsthesia is identified with chloroform. But the discovery of a practicable, safe, and efficient means of insensibility had been made a year before chloroform was thought of, and nothing important has been added to it since. Chloroform is at a first inhalation of an agreeable odour, more portable and less inflammable than ether, qualities which eminently adapt it to army use; but it will do nothing that ether does not do as well, and is sometimes, though quite rarely, it is true, followed by fatal consequences.

We are now to examine the claims of the three individuals mentioned, to the discovery of the new anæsthesia. Let us look first at those of Dr. Wells. And as preliminary to the examination of his claim, let us here revert to an interesting point in this history. It is impossible to read the annexed statement, familiar though it be, without renewed amazement that this great blessing to animal existence was distinctly offered to scientific men, and as distinctly neglected by them for half a century.

The following are the words of Sir Humphry Davy, at the beginning of the present century; half the century had nearly elapsed before they were again thought of:—

> "In one instance, when I had headache from indigestion, it was immediately removed by the effects of a large dose of gas" (nitrous oxyde), ... "though it afterwards returned, but with much less violence. In a second instance, a slighter degree of headache was wholly removed by two doses of gas.
>
> "*The power of the immediate operation of the gas in removing intense physical pain I had a very good opportunity of ascertaining.*
>
> "In cutting one of the unlucky teeth called *dentes sapientiæ*, I experienced an extensive inflammation of the gum, accompanied with great pain, which equally destroyed the power of repose and of consistent action. On the day when the inflammation was most troublesome, I breathed *three large doses* of nitrous oxyde. The pain always diminished after the first three or four inspirations; the thrilling came on as usual, and uneasiness was for a few minutes swallowed up in pleasure. As the former state of mind, however, returned, the state of organ returned with it; and I once imagined that the pain was more severe after the experiment than before. . . . . ."

Towards the conclusion of his book he adds:—

> "*As nitrous oxyde, in its extensive operations, appears capable of destroying physical pain, it may probably be used with advantage during surgical operations in which no great effusion of blood takes place.*"

The great discovery was here clearly pointed out to every tyro in medicine and chemistry. There were three experiments, of a completely original character, and with a new agent, in a direction to which contemporaneous attention was not, as afterwards, leaning. Upon these an original hypothesis was methodically constructed and distinctly enunciated in print. It only remained for somebody to test this hypothesis, this guess, and to convert the guess into a certainty. But neither dentist nor surgeon responded, and the world for nearly fifty years attended exhibitions where people danced, laughed, and became unconscious, while hospital patients were undergoing amputation, alive to all its agony.

In 1844 Horace Wells appeared, exactly repeating the hypothesis that Davy had printed. Whether he got this idea from Davy, or from his friend Cooley, or from his own brain, is not to the purpose. Davy had announced it, and the scientific world knew it. Did Horace Wells convert into a certainty the probability of Davy? He did not. He signally failed to do so, and, mortified by his failure, he gave up all attempts in that direction. More than two years elapsed, and the ether discovery was made and completed. Then, and then only, was Wells stimulated to renewed effort. But it was too late; the discovery of a surgical anæsthetic had been made.

These facts deserve a careful examination. Wells had experimented in about "fifteen cases," and with varying success. This he states distinctly in his first reclamation, before his claim had expanded so as to embrace, as it afterwards did, the whole anæsthetic discovery.

He was "so much elated with the discovery" (to use his own words) "that he started immediately for Boston," and obtained from Dr. Warren permission, as Dr. Morton, imitating Wells, subsequently did with ether, to exhibit the anæsthetic properties of gas before the medical class. Dr. Warren was the principal New England surgeon of that day, and it was the obvious thing to do. This experiment, which was in tooth-pulling, was an utter failure, and was called, as Wells says, "a humbug affair." He was completely discouraged; went home, told his friends that the gas "would not operate as he had hoped," and wholly ceased to experiment, from the date of his failure in Boston, in December, 1844, until the spring of 1847, after he returned from Paris, an interval of more than two years. Wells's want of success can now be satisfactorily explained. He had, through Colton, in following Davy's instructions, made use of the traditional exhilarating-gas-bag, and of Davy's exhilarating dose. This volume of gas is inadequate to produce anæsthesia with any certainty; and Wells failed to suggest a larger dose. This small omission closed his chances. He narrowly missed a great invention. Inventors, by thousands, have missed inventions as narrowly.

Modern dental insensibility by nitrous oxide is unfailing, because the volume employed is much larger. It is also usual to exhale it into the atmosphere. Horace Wells had no hand in this method, of which the first demonstration was in a breast excision performed by myself at the Massachusetts General Hospital, in April, 1848, by means of about sixty gallons of gas, as a substitute for ether.

From all this it will be seen that Wells did not, as has been claimed for him, "discover that the inhalation of a gaseous substance would *always* render the body insensible to pain during *surgical operations*," but only that it would *occasionally* do so; and until long after the ether discovery, his experiments were *not "surgical operations," but only tooth-*

pulling. Wells's anæsthesia had no value to patient, dentist, or surgeon. In endeavouring to trace dental anæsthesia, as Davy had directed, from toothache to tooth-pulling, his experiments unfortunately and erroneously showed that what availed in Davy's hands for toothache would not always avail for tooth-pulling. His slight, but fatal, error of using an inadequate volume of gas damaged the knowledge of his day; so that a scientific person who had read Davy's encouraging and unqualified prediction, based upon his three successful experiments, would have been discouraged and thrown off the track by witnessing Horace Wells's contradictory results.

Wells returned from Boston to Hartford. Having hoped much from anæsthesia as a money speculation, he now left it for more promising enterprises. He got up a panorama or exhibition of Natural History at the City Hotel at Hartford; initiated an extensive business in the sale of patent shower-baths; and somewhat largely invested in cheap copies of Louvre pictures painted in Paris, to be framed and sold by auction in this country. This carried him to Paris about December, 1846; an important event in his career, as we shall presently see.

Before he went, however, the ether discovery was made (October, 1846), and he received from Morton the following letter:—

"BOSTON, October 19, 1846.

"FRIEND WELLS. Dear Sir: I write to inform you that I have discovered a preparation, by inhaling which a person is thrown into sound sleep. The time required to produce sleep is only a few moments, and the time in which persons remain asleep can be regulated at pleasure. Whilst in this state the severest surgical or dental operations may be performed, the patient not experiencing the slightest pain. I have perfected it, and am now about sending out agents to dispose of the right to use it. I will dispose of a right to an individual to use it in his own practice alone, or for a town, county, or State. My object in writing to you is to know if you would like to visit New York, and the other cities, and dispose of rights upon shares. I have used the compound in more than one hundred and sixty cases in extracting teeth, and I have been invited to administer to patients in the Massachusetts General Hospital, and have succeeded in every case.

"The Professors, Warren and Hayward, have given me written certificates to this effect. I have administered it at the hospital in the presence of the students and physicians, the room for operations being as full as possible. For further particulars I will refer you to extracts from the daily journals of this city, which I forward to you.

"Respectfully yours, WM. T. G. MORTON."

To this Wells returned the following remarkable and conclusive reply:—

"DR. MORTON. Dear Sir: Your letter, dated yesterday, is just received, and I hasten to answer it, for fear you will adopt a method in disposing of your rights which will defeat your object. Before you make any arrangements whatever, I wish to see you. I think I will be in Boston the first of next week, probably Monday night. *If the operation of administering the gas is not attended with too much trouble, and will produce the effect you state, it will undoubtedly be a fortune to you, provided it is rightly managed.*

"Yours, in haste, H. WELLS."

(A fac-simile of this letter is here appended. The original is in the collection of the Massachusetts Historical Society.)

Wells would not have thought that Morton's "operation" of "administering" his so-called "gas" (meaning ether) would prove "a fortune" to him, if his own results of two years before had, in the opinion of himself or anybody else, any considerable anæsthetic value.

The rest is briefly told. Wells soon after sailed for Europe, to prosecute the picture business already mentioned. The distinguished American dentist, Brewster, resident in Paris, hearing of his brother dentist Wells, sent to him, "begging him to call on him," and asked him, "Are you the true man?" "His answers, his manners," writes Brewster, "convinced me that he was." "*Dr. Wells's visit to Europe,*" writes Brewster again, "*had no connection with his discovery*, and it was only after I had seen the letters of Drs. Ellsworth and Marcy that I prevailed upon him to present his claim to the Academy of Sciences, the Academy of Medicine, and to the Parisian Medical Society." The quarrel of Jackson and Morton was Wells's opportunity, and Brewster's persuasion thus secured for him a European hearing.

Thoroughly uneasy, Wells returned home (March, 1847). The world was everywhere ringing with ether announcements. From this period his claim rapidly expanded, until it embraced the whole discovery, unsettled his business relations, embittered his life, unhinged his reason, and he at last died, in New York, a sudden death, after extraordinary acts which led even to his arrest, but for which he could not be considered responsible.

Thus perished Wells, volatile, ingenious, enterprising; an experimenter, like scores of others, in the field of anæsthesia, but, like them, unsuccessful in establishing anything of value. So far as his labours went, he left scientific knowledge, as well as its application to art, just about where Davy had left it half a century before. But he had kept the subject alive, and had unintentionally planted a seed in the mind of his ambitious partner which yielded fruit.

We now come to the claims of Dr. Jackson and Dr. Morton; and these, for convenience, we will consider together.

It is significant that Wells, Jackson, and Morton were all in contact at some period of their anæsthetic experiences, of which they shared in some degree a common knowledge. Wells, while in Boston, visited Jackson's laboratory; and Jackson says that he knew of Wells's experiments; and it should be observed that his advice to Channing and Peabody was after Wells's visit. Morton had been Wells's partner in dentistry, and boarded at Jackson's house In 1844 he had been a student of Jackson, who testifies in a certificate that he was a "skilful operator in dentistry," and had "studied the chemical properties of the ingredients required for the manufacture of artificial teeth."

Gas and ether were long familiarly known to produce effects so similar that whoever thought of one as an exhilarator or anæsthetic must have thought of the other. For example while in College, in 1837, I twice made a gasometer of nitrous oxide, and then substituted for it ether, as affording equal exhilaration. Brewster, in 1847, said, "It required neither a physician nor a surgeon to tell . . . . that ether would produce insensibility, ... as there is scarcely a school or community in our country where the boys and girls have not inhaled ether, to produce gayety, and many are the known cases where they became insensible." In short, gas and ether experiences were very similar. Wells had been, at the outset, distinctly advised to try ether, but elected gas as less dangerous. If, when afterwards "disheartened by the failure" of his gas experiment in Boston, he remembered ether, he doubtless thought it would be hardly worth his while to recur to an agent so similar in its effects.

In September, 1846, Jackson and Morton had their well-known interview. At this interview Jackson made a suggestion which was soon followed by a discovery, and by a controversy concerning the value of this suggestion. Jackson claimed that the suggestion was the whole discovery. Morton took the extreme opposite ground in behalf of his experiments. These he alone had conducted, and, while Jackson beyond all question kept aloof, he, recognizing their generally conceded danger, had gone on with them, notwithstanding, and proved what was before only suspected.

The interview was briefly this; and as it is the only point at which Jackson touches the progressive line of Morton's investigation, it should be stated strongly for Jackson. On the 30th of September, 1846, Dr. Morton went to the laboratory of Dr. Jackson, whom he knew well, having been a student under him, and recently in his house, and took from a closet an India-rubber gas-bag. In reply to an inquiry of that gentleman, he said, in substance (and all that Jackson claims Morton to have said may be admitted), "that he meant to fill the bag with air, and by its aid extract the teeth of a refractory patient."[1] A conversation ensued upon the effects of the imagination, also, among other things, of nitrous oxide, in producing insensibility. Jackson treated Morton's proposition lightly. He told him to go to an apothecary and procure sulphuric ether—the stronger, the better—which would produce the insensibility he desired. The ether was to be spattered on a handkerchief and inhaled; in a moment or two perfect insensibility would be produced. "Sulphuric ether," said Morton, "what is that? Is it gas? Show it to me." Jackson showed him some ether, and after further conversation Morton went to procure it. Such was the substance of the interview at which Morton professed ignorance of ether, and Jackson entire knowledge of it. Jackson's knowledge and Morton's alleged ignorance we may now consider.

Dr. Jackson, if we may judge from his later attitude, was not indifferent to renown; nor was he regardless of the suffering of other people. He claims to have discovered, in 1842, that ether insensibility was infallible, thorough, and safe, and yet he cared so little for his reputation that he did not publish his discovery; and he forgot his humanity so far, that he allowed patients, the world over, to encounter the agonies of amputation during four years more. This extraordinary circumstance cannot strengthen belief in the fact of the discovery at this early date.

In a long communication to Humboldt, in 1851, and in certain other communications to learned foreign societies and individuals, Jackson lays great stress upon the elaborate mental process which enabled him to construct, in 1842, an hypothesis of insensibility, based upon the distinct functions of nerves of motion and sensation, superficial and deep sensibility, etc. But the more studied and complete this hypothesis, and the more circumstantial the evidence of its careful elaboration, the more remarkable is it that it was laid on the shelf for four years. Without questioning the entire honesty of Dr. Jackson's convictions, it is safe to say that it is difficult to measure the original strength of any belief which has lain dormant for four years, especially if that belief has since proved to be a valuable truth. Dr. Jackson claims, indeed, to have mentioned his hypothesis to several persons during this interval; but this testimony, if carefully examined, relates chiefly to a narrative of his chlorine gas experiences. In fact, some of Dr. Jackson's immediate family had, during this same interval, in 1844, submitted to painful tooth-extraction by Morton, and yet no anæsthesia was mentioned then. The hypothesis seems to have had for several years a precarious existence.

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[1] It is to be observed here, that, if the patient was intelligent enough to obey instructions, and if Morton really meant to administer air, the patient would have become insensible by asphyxia. (See p. 166.)

The only striking testimony is that of Peabody, a pupil, who was advised by Jackson to employ ether in having a tooth drawn, but who, after consulting his father, an accomplished amateur chemist, was deterred from doing so by the reputed danger, which Jackson's suggestion did not outweigh. But this advice was a year after Wells's experiments and failure in Boston, and his conversations at that time with Jackson. Whenever tooth-pulling was brought to Jackson's notice, how could he fail to think of Wells's experiments with gas? And who could think of gas as an inebriator, without its co-inebriator, ether? the obvious and only possible conclusion being, that what gas had done, ether might do, namely, sometimes succeed and sometimes fail. I have no doubt that this thought did occur to Jackson's mind when tooth-pulling happened to be talked of, especially after Wells's experiments.

It also occurred to Morton's mind. He knew more of Wells and of his varying experiments than Jackson did, and there is no question that Morton the dentist, oftener than Jackson the chemist, dwelt upon painless dentistry. His business was "mechanical dentistry," making sets of teeth; and he was daily suffering in purse because patients feared to have their teeth drawn. "I will have some way yet," said Morton to Gould, in August or September, 1846, a short time before the discovery, "by which I will perform my operations without pain." "I smiled," replied Dr. Gould, "and told him, if he could effect that, he would do more than human wisdom had yet done, or than I expected it would ever do."

Who that remembers the late Dr. Gould, cautious, accurate, truthful, judicial, the friend and brother scientist of Dr. Jackson, will doubt this sworn testimony? It was Dr. Gould, who, when his wife told him of rumors that Dr. Morton had drawn a tooth without pain, under the influence of something inhaled, exclaimed, "Yes, that can be done; ether will do it." So obvious was the transition from gas inebriation to ether inebriation, from gas insensibility to ether insensibility, in the mind of one who happened to have his attention drawn to the subject of anæsthesia by inhalation.

Morton knew of gas, and of Wells. He was in eager pursuit of anæsthesia. He believed in it. If he knew, also, of ether, he was, in all human probability, on the verge of discovery. Did he know of ether?

It is fortunately established beyond all question that Morton made long inquiries about ether in July, and also, a short time before the October experiments. If we reject the evidence of Wightman and Metcalf on this point, both of them disinterested, accurate, reliable, we must reject all human testimony.

Wightman the philosophical instrument-maker, afterwards Mayor of Boston, narrates a long conversation with Morton in the cars, when moving his family from his country residence to Boston. During this conversation he informed Mrs. Wightman, who asked him who Dr. Morton was, "that he was a dentist who was experimenting upon the relief of pain in dental operations." Wightman fixes the date, September 28th, of this journey, by items of expense charged in his day-book, of which the leaf was produced, as part of his sworn testimony. This enables him to fix the time of several previous conversations with Morton, concerning mesmerism, the effects of the imagination, etc., and especially of one as to whether rubber or oiled silk bags would hold "common ether."

Metcalf, the well-known apothecary, a scrupulously conscientious witness, equally substantiates the date of a conversation half an hour long, about ether, with Morton, who held in his hand a bottle of it which he had brought with him. In this conversation, " entirely about the inhaling of ether," interspersed with anecdotes of exhilaration and insensibility, Metcalf told Morton of "a person to whom he had given it, who was exceedingly wild, and who injured his head while under the influence of it, and did not know, when he got over the influence of the ether, that he had hurt himself, until it was called to his attention." "Morton," Metcalf testifies, "when he went away, knew as much about it as I did, for I gave him all the information which I had." Metcalf sailed for Europe in July, 1846, just after this conversation. While in Italy, in the early part of 1847, he took up a newspaper announcing the discovery, by a Boston dentist, of insensibility through ether inhalation. "I said at once," testifies Metcalf, " that I was sure Morton must be the man, for he was engaged upon ether before I left home ; and that I now knew why he had been so curious, and at the same time shy, in his conversation with me." To those who know Mr. Metcalf this evidence has all the weight of personal experience.

There can be no question that Morton knew about ether. How much he knew about it is less important. But it should be mentioned that he claims to have made repeated experiments with it upon animals in the summer of 1846.

Morton's explanation of his professed ignorance of sulphuric ether was this. During the summer, while boarding at Dr. Jackson's, he had heard frequent and protracted expositions of Jackson's claim to the invention of the electric telegraph, then recent, and the important features of which Jackson was satisfied he had communicated to Morse during an ocean passage. Dr. Jackson had, indeed, a well-stored and suggestive mind, which made it more than likely that he had furnished information, of which Morse, while originating and mentally evolving a system of electric telegraphy, may have been glad to avail himself. A sharp public discussion, with pamphlets, ended with a verdict in Morse's favour. In going to Jackson, Morton feared that if there were any deliberate conference, Jackson might set up a similar claim of participation in his own search for painless dentistry, and therefore took the shortest way to exhaust his knowledge for his own benefit, without discussion. There can be little doubt that Morton was in this matter reticent, as Metcalf states, and intended to keep it a secret from his brother dentists. I am also inclined to believe that Morton at first cared little about the abstract question of discovery, and would have willingly left a large share of any honour unquestioned in the hands of Jackson. But when Jackson made a claim upon the patent, and the profits, beyond the amount to which Morton conceived him to be entitled, then he defined his own claim to the discovery. It may be stated in this connection, that the surgeons of the Massachusetts General Hospital, who had no interest whatever in this difference, and could have none, were friends of Jackson, and strangers to Morton. They yielded, when it became necessary to take sides, only to their deliberate conviction of the justice of Morton's claim.

It will be advantageous, at this point, to take a general view of the "ether controversy." For this purpose, I find I can do no better than to quote the following letter, written, when the occurrences were fresh, to the Hon. Robert C. Winthrop, in Washington:—

January 26, 1848.

DEAR SIR: I believe most fully, that Dr. Morton deserves any reward Congress may grant to the discoverer; because, although many people have *thought* that a man could be intoxicated beyond the reach of pain, Dr. Morton alone *proved* this *previous possibility* to be *a certainty*, and *safe*. A diagram will make the matter plainer than words:—

Before October, 1846, who made the suggestion? Here is the only ground of dispute.	Discovery in Oct. 1846. Consecutive experiments by Morton.	After October, 1846, *Morton alone* took the responsibility of danger, and proved that it was 1st. *Certain.* 2d. *Safe.*

The two last points, namely, the consecutive experiments, and their confirmation, *which nobody disputes to Morton*, make him, in my eyes, the discoverer. The only doubt is, who made the *suggestion? To me this is of no importance.* Dr. Jackson says, "I did. I told Mr. Morton to try the experiment; and unless I had so told him, he would never have tried it." Dr. Jackson adds: "I first tried ether when I was suffering from chlorine, in 1842. I afterwards recommended it to Mr. Peabody." But Dr. Morton confutes even these positions. He says to Dr. Jackson: "1st. I show, by the evidence of Dr. Gould, Mr. Wightman, and Mr. Metcalf, that I was experimenting with ether before the interview in which you claim to have brought it to my notice. 2d. In 1842 you only re-discovered what was before clearly in print in Pereira's *Materia Medica*. 3d. You claim that you told Mr. Peabody what you *knew* of ether. Now you could not *know* it. You have stated all your grounds of deduction, and the widest inference you could draw from them is, a *suspicion* of the properties of ether; and a *suspicion* in science, an *unconfirmed theory*, amounts to nothing. Finally, what you claim to have discovered in 1842 you kept to yourself during four years. Do you expect the world to believe you knew its value? Do you expect it to reward you for letting people suffer during that length of time? Besides, the suggestion of anæsthetic agencies occurred to Davy; especially was it followed out, though unsuccessfully, by Horace Wells, who, disgusted with failure, abandoned his attempts." These and others had hypotheses as well as Dr. Jackson. Morton alone proved the hypothesis. Without Morton there is no evidence that the world would have known ether to the present day. I believe this covers the ground of important argument and difference in the pamphlets. . . . .

Respectfully your ob't servant,

HENRY J. BIGELOW.

At the interview referred to, Jackson's partisans tenaciously insist that he assumed direction, as a physician might have done, of the administration of a remedy; while Morton acted only as a "nurse." Let us examine, then, the often-quoted "nurse" argument with which the opponents of Morton have endeavoured to handicap him at the outset. Here is its fallacy. A physician, by common understanding, possesses a positive knowledge of the effect of his remedies, in advance of their administration. Such knowledge was impossible to Jackson. Again, a physician is employed expressly to direct, and a nurse to obey. Under these circumstances a nurse is not likely to get much credit for originality, which is in fact absolutely excluded by the terms of her contract. There was no such contract here. Morton was not a mere agent, without preconceived plan, automatic in action, and irresponsible as to results. On the contrary, he was already conducting an independent investigation. He was in pursuit of anæsthesia when he went to the laboratory of Jackson, with whom the subject, even admitting his claim, had slumbered for years. He had been before, and in the same way, to books, apothecaries, instrument-makers, in short, to various usual and available sources of knowledge, as is customary with every investigator or discoverer. The purpose, the investigation, the patient, the discretion, the responsibility, were all his. Morton was not a "nurse."

Morton had a combination of qualities such as few other men in the community possessed. Fertile in expedients and singularly prompt in execution, he was earnest and persevering beyond conception. His determined persistence is remembered even at this interval of time, as having been a terror to his best friends. Nobody denies that Morton, recklessly and alone, faced the then supposed danger attending ether stupor. If all accredited scientific opinion had not been at fault, and in the case of any fatal result, he would have infallibly been convicted of manslaughter, with little probability that anybody would have come forward to say, "The responsibility is not his, but mine."

In fact, Dr. Jackson endeavoured at this time, by word and deed, to keep both himself and his reputation clear of Morton, as a reckless and dangerous experimenter. The only operations under ether witnessed by him during the first three months were on November 21, 1846, and January 2, 1847; and a part of this time he was absent from the State.

"But," it has been a hundred times said, "Jackson made a suggestion, and Morton used it." It is evident that we cannot escape some discussion of the relation of "suggestion" to "discovery," no matter how little the suggestion may be intrinsically worth, or how fortuitous its success. The simple question is, What was the actual value of Jackson's suggestion to Morton at the time it was made—I distinctly mean before Morton had handled it?

A suggestion in science varies in value as much as a suggestion in common life, where advice is not always sound advice. It may, indeed, turn out fortunately, like a suggestion, for example, to wager money on the ace of spades. But because it may prove so, the advice does not necessarily imply merit in the adviser. This is an extreme case. At the other extreme is a mathematical certainty, such, for example, as that twice two make four: a truth the value of which is not afterwards augmented for intelligent people by a material test or demonstration that twice two apples make four apples. A similar example of mathematical certainty is that calculated by Leverrier (liable only to the instrumental fallibility of dividers, telescopes, and equations), which did not become more absolute, nor more true, when Galle saw the planet where Leverrier told him it must be. Mathematics are unerring, while predictions in physiology are as uncertain as predictions about the weather a week hence. Yet it has been argued that Jackson was Leverrier, and Morton Galle.

Jackson's alleged hypothesis, before Morton took hold of it, had only the value of a lottery ticket, which, through Morton's unaided, dangerous, and acknowledged efforts, drew an immense prize—or of "the cast of a die"

—"for in that light," says Watt, whose name is identified with the history of steam, and the soundness of whose practical views no one will dispute, "I look upon every project that has not received the sanction of repeated success." A statement of the grounds upon which this view is based will enable others to draw their own conclusions. Let us begin with Jackson's experiments.

"Having, in 1841–2," says Jackson, "got my lungs full of chlorine gas, which nearly suffocated me, I immediately had ether and ammonia brought to me, and alternately inhaled them, with great relief." The next day, still suffering, and "perceiving a distinct flavour of chlorine in my breath," the experiment was repeated "with perfectly pure washed sulphuric ether," and "with entire loss of feeling." "All pain ceased." He "fell into a dreamy state, and became unconscious of all surrounding things."

In other words, he inhaled ether, until, seated as he was "in a rocking-chair, with his feet in another chair," he fell asleep. So far there was nothing new. Pereira, for example, says (*Elements of Materia Medica*, etc., London, 1839, pp. 210–211):—

> "The vapour of ether is inhaled in spasmodic asthma, chronic catarrh, and dyspnœa, whooping-cough, and *to relieve the effects caused by the accidental inhalation of chlorine gas.*" "When *the vapour of ether sufficiently diluted with atmospheric air* is inhaled, it causes irritation about the epiglottis, a sensation of fulness in the head, and a succession of effects *analogous to those caused by the protoxide of nitrogen* (vide p. 156), *and persons peculiarly susceptible to the action of the one are also powerfully affected by the other.* (*Journ. Science*, vol. iv. p. 158.) *If the air be too strongly impregnated with ether, stupefaction ensues.*"

Such was contemporaneous knowledge. Jackson's experience was identical with that recorded by Pereira. And Pereira further distinctly calls attention to the similar effects of inhaled gas and inhaled ether, and to the stupefaction caused by ether.

Pereira adds, in regard to the danger of this condition:—

> "In one case this state continued, with occasional periods of intermission, for more than thirty hours; for many days the pulse was so much lowered that considerable fears were entertained for the safety of the patient. (*Op. cit.*) In another case, an apoplectic condition, which continued for some hours, was produced."

I shall revert to this subject of danger.

But Dr. Jackson alleges that he now advanced a step further, and logically inferred the entire safety and inevitable certainty of ether anæsthesia, in all cases, and during the severest surgery. This was an unwarrantably wide inference, as we shall see.

Further still, Dr. Jackson claims to have invented a method upon which the success of his hypothesis largely, as he supposed, depended; and he offers this method as evidence that he made the hypothesis and the discovery. It is plain that a discovery may be in this way rendered more probable. Whoever is in possession of a new method is more likely to find the way to a new truth. But if the alleged method proves to be partly erroneous and partly an old and familiar matter, then the proof of alleged discovery is no stronger because of it. In fact, just so far as the hypothesis was apparently stronger by reason of the method, it becomes weaker when the method falls to pieces. Let us, then, examine this alleged method.

Dr. Jackson has from the first insisted upon two points, as peculiar to his invention, and essential to his discovery. By these safety and certainty are secured, and a neglect of them explains previous uncertainty and danger. They are,

1st. Purity of the ether;

2d. An admixture with it of atmospheric air.

If these conditions are either not essential or not new, we find ourselves, at least so far as method is concerned, where Pereira left us.

From time immemorial, the familiar way of inhaling ether has been with air from a handkerchief. Pereira, as before quoted, distinctly stipulates for air. Says another writer, "Animals breathe oxygen. Without oxygen a man must die. Ether would not have saved Desdemona." This needs no ghost for its enunciation. But the fact is, in etherizing by a sponge or cloth, the difficulty is as often to exclude air enough as to admit air enough. Some good authorities maintain that there is advantage in its exclusion—that the insensibility from gas, for instance, is due to asphyxia. Even in etherizing, they aim to take advantage of this condition by restricting the air supply. The French ether-bag, still in use, is expressly arranged for this purpose. In short, while it requires especial effort to exclude air, partial asphyxia is not dangerous; and a claim to the discovery of the safety or certainty of ether stupor based upon the admission of air cannot stand.

A claim based upon pure ether is equally void. Tolerably pure ether is better, but by no means essential, for safe insensibility. The specific gravity of pure ether is not very far from 0.718, and is difficult to attain; that of our usual inhaling ether of Powers & Weightman, 0.724; and of Squibb (*fortior*) about the same. The ether of the old Pharmacopœia, and of the shops in the year 1840, was not far from 0.750; and this is a very practicable anæsthetic. Its slight adulteration with alcohol and acid is not especially deleterious, when inhaled. In fact, anæsthesia resembles dead-drunkenness, which is equally possible with brandy, or with brandy and water. The real danger was not from impure ether, but from over-inebriation—a danger which exists to-day with the best ether.

The discovery was not in the admission of air, nor in the use of a particular quality of ether. It was, that the inhalation of ether, which had been familiarly resorted to for years, could be prolonged beyond the usual stage of exhilaration to a stage of stupor, possessing the *complete* insensibility of a dangerous coma, yet, unlike that condition, *safe;* and that all this could be effected *in every case.*

The history of ether anæsthesia was the gradual discovery of the following facts:—

1st. That ether inhaled produced, capriciously, in certain instances, unconsciousness. (Old.)

2d. That it possessed the power of producing stupor in every case. (New.)

3d. That this stupor could be exactly graduated as to time. (New.)

4th. That it could be increased or diminished, and arrested short of danger, and so made safe. (New.)

5th. That there were certain infallible indications of danger. (New.)

6th. That, while thus controllable and safe, it could be made at will so profound that even amputations should not be felt. (New.)

All that is new belongs to Morton.

A person inhaling ether from his handkerchief would drop it when his hand and senses were benumbed. As air entered the lungs he would revive. Such was Jackson's self-experiment, already covered by Pereira. But a second person, who should now seize the handkerchief and compel the continued inhalation to the stage of stupor, might obviously make a valuable study of this new ground. He could draw his patient's tooth, or amputate his leg, and thus measure the insensibility. He could repeat the experiment until satisfied that insensibility could be always attained, and that it was safe. All these experiments Morton tried upon others, and when he had tried them the discovery was made.

Jackson virtually claims that his inference extended to the *universal efficacy*, the *completeness*, and the *safety* of the stupor.

Of the *universal efficacy* of the new stupor Jackson could have no valuable opinion. Such knowledge was possible only through observation of many cases, with an experience which could say, "Administer ether as for exhilaration. Protract its inhalation beyond this usual stage, and you will inevitably reach an ulterior condition of stupefaction. During this process a patient may struggle with a giant's force; or perhaps will tell you of his extreme distress; or after five or ten minutes of inhalation will satisfy medical bystanders, as patients sometimes do now, that he is not amenable to ether. Another will assure you, with apparent reason, that he is dying; or, livid with asphyxia, will compress his lips and cease to breathe; or may, for a long time, alternate between lividity with a failing pulse, and arterialization with a partial return of consciousness.[1] But, through all these apparently alarming indications, urge the process discreetly, and you will ultimately and inevitably reach a stage of stupefaction."

Dr. Jackson could not say all this, because he knew nothing of it; nor did anybody else, until Morton established the fact that there was no exception to the potency of ether. These and other contingencies, once startling, still occur with the best ether, and the experience of a quarter of a century. Had Morton been a timid or a discreet man, anæsthesia might have been delayed beyond the present generation. Morton compelled inhalation in spite of indications to arrest it, incurred the responsibility of doing so, and is entitled to the credit.

The completeness of the insensibility—its adequacy, for example, in amputations—was settled only by repeated observation, with varying results, but gradually accumulating evidence: from the tooth first painlessly extracted, through several failures to comparative success in dentistry; then the removal of a venous tumour of the jaw, where the patient was doubtfully insensible; then a fatty tumour of the arm, with complete insensibility; and finally, after an inconsiderable operation or two, the amputation of a leg, practically successful. Everybody awaited this final test by amputation, and then only was the accumulated evidence deemed conclusive. It was, indeed, beginning to be felt that the process was a safe one, and that it promised satisfactory results; otherwise the hospital surgeons would not have permitted this test of it in a patient weakened by disease. But this experiment, like the rest, so far as anæsthesia was concerned, was Morton's, and a part of his well-organized enterprise to investigate and establish the new insensibility. As this amputation has been repeatedly published as mine, it should be stated that it was performed by the late Dr. Hayward. That anæsthesia was employed at all on that occasion

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[1] Such patients are familiarly known to our hospital attendants as "bad etherizers." Some of them are inebriates. If they return for a second operation, they seldom fail to exhibit the same symptoms as before.

Alice Mohan has to thank me; although, if anæsthesia had not been employed in this particular instance, the test of amputation would have been delayed only till another should occur. The circumstances may illustrate the sort of obstacles Morton encountered. The looked-for opportunity had arrived; but, through various antagonistic influences, Morton, in spite of his earnest request, had been notified that ether would not be administered, and that he need not attend the operation. Of this he informed me the night before, and by arrangement with him I carried him to the hospital the next day, just before the operation, there to await events. A dose of laudanum had been administered, and Alice Mohan was carried to the amphitheatre, for operation without ether. I there strongly urged the employment of the yet ostensibly secret agent; partly on the ground that it then was really known, but especially from the consideration that humanity ought to supersede any doubts connected with professional etiquette. This and other considerations prevailed, and, after a delay of half an hour, Morton, whose presence had been till then unknown except by me, was brought up, and the patient was etherized.

The evidence of all this slowly accumulating anæsthetic surgery, at which Dr. Jackson was not present, was claimed for him in virtue of the "nurse" argument. What light could a repetition of the chlorine experiment of Pereira throw upon the question whether a patient could sleep during an amputation?

Ether exhilaration was familiar; but, on the other hand, it was well understood that ether stupefaction was in certain cases dangerous. Physiologists had also found that the smaller animals very frequently died of it. Brodie wrote of the fresh discovery: "I have heard of this before, and had tried it on guinea-pigs, whom it first set asleep and then killed. The great question is, Is it *safe?*" This was, indeed, the question. Could danger be avoided? what was its exact character? and what were its indications?

On the second of January, nearly three months after the discovery, Jackson came to the hospital for the first time. To that date, he had been present at one operation only, that at the Bromfield House, November 21. He was not cognizant of current experiment, and brought voluntarily a bag of oxygen, which he urged upon the hospital surgeons as a necessary precaution against danger, erroneously supposed by him, at that late date, to be asphyxia, instead of over-inebriation, of which the essential indication is the pulse. It was some weeks after the discovery, that this source of danger, and its sign, were understood; and in the mean time Morton might have killed anybody. A patient was, in fact, in great danger from over-inebriation at the first private operation. He was inhaling, in the continuous way that was at first supposed to be essential to protracted insensibility, through a glass globe of ether, and long after insensibility was manifested. The operation was far from completed, when a bystander happened to feel the pulse. There was no special reason for doubt, inasmuch as the patient was, in general appearance, like all former thoroughly etherized patients. The pulse proved to be barely perceptible, and the patient to be etherized almost beyond recovery. The bystander, after repeated observation of other cases, published the fact, then first observed, that in ether anæsthesia the pulse stood as a beacon between safety and danger, between harmless inebriation and fatal narcotism. This was the discovery that ether was not dangerous; because this showed that its danger gives warning, and is under control. The operator was Dr. Dix, the bystander myself, and the discoverer Morton. To his impetuous, unremitting, reckless experimentation to establish anæsthesia, surgeon, bystander, patient, ether, and apparatus, were all for the time, and in that relation, subordinated. Morton had asked me to be present, because I was more familiar with the new process than anybody except himself, and for the purpose of aiding him, in emergency, with professional advice. But the anæsthesia was his. I assumed no responsibility. Had the patient died in a "stupor," as he might well have done, Morton was liable; and as the patient did not die, his was the credit. This was real danger. But there was other danger, more startling, though only apparent; such as prostration, "trance," or "mania," lasting for hours, and for which Morton was in one instance threatened with prosecution. What was Dr. Jackson's responsibility? None whatever. He was then absent from the State. What had Pereira's chlorine experiment taught him about all this danger? Absolutely nothing; nor could it do so. And he could impart nothing.

In view of these facts, which leave Jackson standing upon the naked experiment of Pereira, we may fairly pause, and ask, What, in the fullest sense, were the exact significance and value of the suggestion made by Jackson to Morton?

Caleb Eddy says, in his sworn testimony, "I said to Dr. Jackson, 'Dr. Jackson, did you know, at such time, that, after a person had inhaled the ether and was asleep, his flesh could be cut with a knife without his experiencing any pain?' He replied, 'No, nor Morton either: he is a reckless man for using it as he has.' "

Waiving this testimony, it is clear that the whole of the peculiar knowledge embraced in the suggestion of Jackson to Morton would have been accurately and fully conveyed in the two words, "Try ether." This suggestion should be fully credited to Jackson. Morton never questioned the fact of the suggestion; on the contrary, he at once proceeded to square the account. Jackson at first distinctly agreed to receive five hundred dollars, as full compensation for the assistance he had rendered. As the evidence grew, and the greatness of the discovery became more apparent, Jackson raised his demand to ten per cent. on the sales of patent rights. Later, when it was clear that the lottery ticket had drawn an immense prize, Jackson again increased his demand to twenty-five per cent., which Morton refused. The controversy was then opened.

I very early urged the inexpediency of a patent, if only on the ground that, like Whitney's cotton gin, for example, this invention was so valuable that the world would combine, as the event has shown, to take possession of it; and that the question of equivalent might safely be left to public generosity, which has generally recognized such debts. Ether anæthesia was at first opposed on the ground of its danger, of quackery, of religion, and of professional etiquette. Much of the early opposition to a patent[1] came from dentists, who desired to use the new method without pay; and they confused with it the question of humanity to suffering. But in those days dentists had secret methods to which they attached a money value, and which went far to justify both secrecy and patent. The question of secrecy should, however, be detached from that of equivalent. Dentists and physicians, lawyers and clergymen, dealers in food, heat, light, labour-saving methods, in short, in comfort, knowledge, and value, rightly exact a pecuniary equivalent, proportioned to their services, from those who can pay it without inconvenience. The more distinctly this is recognized, the better we shall understand the nature of real humanity, and the more readily lend assistance and charity to those who need them. Jackson was right in expecting a money return for the service he had rendered. The only question here is, How much he himself, at first, considered a fair equivalent for this service. This has been shown.

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[1] The first statement of the fact of operations under insensibility was a paper in the *Boston Medical and Surgical Journal*, Nov. 18, 1846, entitled "Insensibility during Surgical Operations, produced by Inhalation. Read before the Boston Society for Medical Improvement, Nov. 9, 1846, an Abstract having been previously read before the American Academy of Arts and Sciences, Nov. 3, 1846, by Henry Jacob Bigelow, M.D., one of the Surgeons of the Massachusetts General Hospital." A copy of this was sent by a gentleman to his friend, Dr. Boot, of London, and by him transmitted to several London surgeons. Their replies to Dr. Boot I have in my possession. This paper also announced the patent, and the connection with it of the names of Morton and Jackson.

After it became evident that the patent was worthless, Jackson repudiated the division, and claimed the whole discovery, in virtue of the "nurse" argument. Under these circumstances Morton properly insisted that the "suggestion" could have been picked up from almost any source, by any man actively searching for painless dentistry, who knew everything about Wells's experiments with gas, and who also knew the familiar and similar action of gas and ether. Morton was right.

But Morton also urged that this was a discovery not in science, but in art; that surgical anæsthesia was due, not to any great scientific novelty in the long recognized ether insensibility, but to his having worked out the application of this insensibility to use in art, with enterprise and perseverance, through many details, in the midst of danger, till he gave to the world a perfected system of efficient and safe anæsthesia. Morton was again right.

When a discovery is great, not from the intellect invested in it, but because it ameliorates the condition of mankind, then its recognition has more of a business character, and the gratitude and honour bestowed by the world are more nearly an equivalent for value received. The world does not concede the equivalent until it has received the value; and it is apt to examine with business exactness the claims of those who profess to have acted as agents in the matter.

This suspicion of discoverers who do not appear until after the world has been made to recognize the discovery is justified by the fact that hardly an invention of importance was ever made known that it was not at once claimed, often simultaneously, from a variety of sources. This is not remarkable. The world, whether in science or in art, is built up to a certain point by the easy and wide transmission of knowledge. Upon this elevation stands a multitude of philosophers, engaged often in identical researches, and possessed of much information upon the subject in question. Each of these may honestly believe that his imperfect knowledge is the perfected knowledge in question. In such a case the world is liable for a short time to confound claims, to confuse the incomplete result of a few data with the completed demonstration from many, the unproved with the indisputable, theory with fact. Recognize this fallacy, and the question of invention is comparatively simple. Yet it is not recognized. There is at this day the same claim to priority in invention as existed half a century ago. The writer of a *Life of Fulton* then said: "Those who question Mr. Fulton's claim are precisely those who have been unsuccessful in their own attempts; and it would seem that exactly in proportion as their efforts were abortive, and as they had thrown away money in fruitless experiments, their claims rose in their own estimation and that of their partisans." The witness—I believe before the House Commons—probably did not overstate the matter when he gave it as his opinion, that, if a man were to show that he had found a road to the moon, his neighbours would testify, that, if they had not been there themselves, they knew several persons who were familiar with the road in question. It is hardly too much to say, that, at the present day, every invention or discovery having a large supposed value is systematically contested.

Morton, in his attitude of investigator, had a right to receive a hint or suggestion of greater solidity than that of Jackson, without impairing his merit as inventor. Every invention is preceded by such hints or suggestions derived from experiment, books, or people. Curtis (on Patents) says: "It is clear that many suggestions may have been made, or many hints taken from others, without invalidating the claim of a party to be considered as the author of the invention."

Of a hundred instances easily cited to illustrate this, let us confine ourselves to a medical one, that of the invention of vaccination to avert smallpox.

The young countrywoman of Sodbury said of smallpox: "I cannot take that disease, for I have had cowpox." The Duchess of Cleveland, when Lady Mary Davis and other companions taunted her as likely to deplore the loss of that beauty which was her boast, as the smallpox was then raging in London, said that she had no fear about her beauty, for she had had a disorder which would prevent her from ever catching the smallpox. Were these discoverers? Surely, yes, if Dr. Jackson was one. In fact, the hint that they and others gave to Jenner in the vale of Gloucestershire, where he resided, embodied more knowledge of vaccination than Dr. Jackson's suggestions did of ether anæsthesia.[1] But neither the milkmaid nor the Duchess of Cleveland was ever honoured as a discoverer of truth, or an inventor of method, while Jenner was so honoured. The reason is obvious. They, like Dr. Jackson, asserted a doubtful fact, and had neither time nor inclination to pursue the subject; but Jenner, by multiplying instances of the cowpox inoculated like smallpox, which was already supposed to be, like smallpox inoculation, protective, conclusively proved that it was thus protective, and also safe. He did with cowpox what Morton did with ether.

The parallel in this case is very close. Jenner and Morton were in pursuit of what the scientific world regarded as a chimera. Because they believed in its possibility they encountered prejudice and opposition. They both received from others a hint, suggestion, or surmise, which afterwards proved to be capable of development into a truth of great value. This suggestion was based on narrow induction, and had, therefore, obtained no previous general credence. It had also slumbered for years at that stage of its development. The world had not believed it.

Jenner and Morton, both men of singular persistence and perseverance, took hold of this idea, of which, from the nature of their daily pursuits, they felt the immense value. But for them it might have slumbered indefinitely. They dragged it through till the world recognized it and them. This they did at the risk of injuring people's health, of killing them, and of being held responsible for so doing. Nobody has ever doubted that Jenner was the inventor of vaccination, and nobody should doubt that Morton was the inventor of modern anæsthesia. Here the parallel ceases. The English people voted Jenner a reward of $150,000.[1]

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[1] The experiences of the milkmaid and the Duchess might easily have apprised them of their own immunity from smallpox. But Jackson, through his chlorine experiments, could have no evidence that ether stupor was capable of affecting all persons, or that it gave immunity from real surgical pain, or was free from danger. No self-experiment could touch these points.

The world demands convincing demonstration—and not by an individual for himself or to himself, but to them and for them, with overwhelming clearness. Then, and not till then, it responds with acknowledgment, concession, or gratitude.

Sydney Smith, in the *Edinburgh Review*, insists on this. In fact, he wittily overstates the claim of the mere publisher of a novelty, when he says that "he is not the inventor who first *says* the thing, but he who says it so long, loud, and clearly, that he compels mankind to hear him."[2]

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[1] In decisions relating to discovery, unanimity is not to be expected. There can be none where partisanship and large interests are involved. The question, then, is, Where the weight of evidence lies. Even Jenner, with no rival, encountered great hostility both to himself and his discovery. The House of Commons (June 2, 1802) voted him ten thousand pounds by a vote of 59 to 56, a majority of three only. A further sum of twenty thousand pounds was voted (June 29, 1807) by a vote of 60 to 47, a majority of thirteen. Morton's award of $100,000, for his patent, passed the Congressional Committee. It was arrested, not wholly by Jackson, but by the partisans of Horace Wells, who published what afterwards filled an octavo volume, containing little argument, but full of bitter invective against Morton, and promised, if only delay were granted, to make a conclusive case for Wells.

[2] It is easy to understand what is meant here. For instance: I have amputated more legs after applying a tight bandage from foot to hip before tightening the tourniquet, than in any other way. Similarly to the arm for a needle in the hand. A hundred surgeons have done the same thing Sir Charles Bell went a step further, and announced the dry method in print. "I may here observe," he says, "that by the management of the tourniquet, blood may be lost or gained. If the limb be uniformly rolled before amputation, the veins are emptied into the general system, and blood is saved instead of being withdrawn. In a very exhausted state of the patient, it may be of service to attend to this." (*Great Operations of Surgery*, London, 1821, p. 58.) But Esmarch was so impressed with its importance, that he erected it into a system, and urged it upon the attention of every surgeon in the civilized world. To many the idea was new. In fact, to a considerable part of the surgical world Esmarch was the discoverer of an important truth. He deserves to be avowed as such, in requital of his pains to perfect and to publish to the surgical world. a useful point, new to many of them. He is so recognized.

Antiseptic precautions in surgery are not new; but Lister published his views, as did Esmarch. Whether germs are essential to the theory or not, there can be little doubt that it is well to free a wound from coagula. and to wash it out with diluted carbolic acid or its equivalent. Then the subsequent free use of the antiseptic. by hindering decomposition without, tends to maintain vitality within the wound. These views have been enforced and brought home to the world by Lister, with the pertinacity of Jenner or of Morton. The world at large owes its attention to these points to Lister's announcements, and properly attaches his name to the antiseptic method.

Mere publicity is notoriety; but with merit it is fame; with originality it is discovery. To all these publicity is essential. He who keeps his discovery comparatively to himself discovers, or uncovers, nothing. If he claims to have done so, the world will scrutinize and suspect his claim.

The act of publication, indeed, adds little to the claim of Morton; his impregnable position being that he elaborated and perfected a new art. But facts like the above go far to show that the failure of Jackson for four years to publish his alleged guess, of itself extinguishes any claim to its recognition during that time.

Thus put, however, the statement throws light on Jackson's prominence after the discovery. An unproved hypothesis in physiology, which was his whole claim, would usually be considered of little account. But Jackson knew the machinery of fame. As Wells by accident, so Jackson by his scientific relations, and through his friend and former teacher, Elie de Beaumont, got at once possession of the scientific rostrum of the French Academy of Sciences and other foreign learned societies. He also subsequently addressed Humboldt. He thus compelled the European world, even so far as Turkey, to listen to his exclusive statement that the whole was his. In the mean time, however, there was in Boston a scientific jury of the vicinage. When Morton's statement at last crossed the water, the French Academy did what it could at that late day, and awarded to Morton the same honour and recognition it conferred upon Jackson. It could do no less. It could then do no more. But had the case been at the outset reversed, and had Morton made a suggestion to Jackson, does anybody doubt that the humbler Morton, and his suggestion, would have been by scientific precedent wholly absorbed and assimilated ?

All honour, then, to the inventor of the art of anæsthesia!—for there was little science in it. He found the practice of ether inhalation an amusement of chemical lecture-rooms and schools ; he left it the sovereign anodyne of the human race in its moments and hours of agony. He found ether stupor as hazardously uncertain as was the narcotism produced by pouring down the opium "*à boire*" of Canappe ; he left it as manageable and safe as the sleep that follows a dose of laudanum.

There is hardly an inhabitant of the civilized world but can remember some one of those nearest to him in whose experience the anguish of the knife or of disease, of birth or of death, has been assuaged by anæsthesia, perhaps converted into a pleasant dream. Yet he is willing to take this priceless boon as a gratuity from those whose sole patrimony it was, and who have been brought nigh to want that he might enjoy it. If the world should cancel a fraction of its debt, the family of the inventor could afford to be generous to the families of his former friends, who, without impairing his title to the discovery, contributed to his success.

Wells's sad story has been told. Morton fell with apoplexy, induced by a publication in behalf of Jackson, of a nature to prejudice a subscription then arranged in New York for his benefit. Jackson, it is to be feared, is at the present time hopelessly bereft of reason.